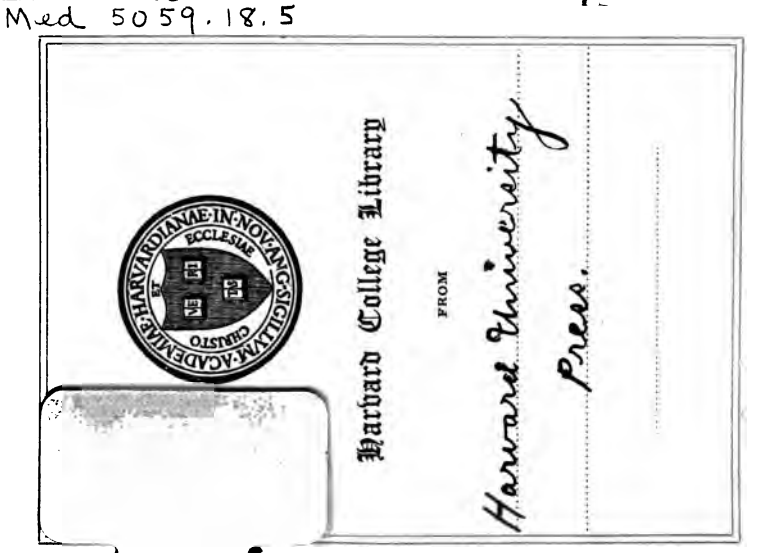

THE EFFECT OF DIET ON ENDURANCE

By

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PREFACE

THE GREAT WAR has given a new and sudden impetus to the fast-growing interest in nutrition and has created, incidentally, a modest demand for the essay of which this little book is a slightly abbreviated reprint.

Since the appearance of the original edition,[1] long since out of print, many studies have been made tending to justify its conclusions[2] and none, so far as I know, tending to discredit them.

[1] "The Effect of Diet on Endurance," Publications of Yale University, *Transactions of the Connecticut Academy of Arts and Sciences*, Vol. XIII, pp. 1-46, May, 1907.

[2] Besides the great mass of technical literature by physiologists confirming the "low-protein" idea, I may cite as reviewing and testing, in a practical way, the conclusions of this book: "The Effects of a High and Low Proteid Diet on Physical Efficiency" by Elmer Berry of the Springfield Y. M. C. A. Training School, *American Physical Education Review*, May, 1909. For an experiment in a financially economical diet, see also "Food" by Dr. Eugene L. Fisk of the Life Extension Institute, 1917.

A decade ago there was little appreciation of the most elementary facts and concepts of dietetics. Today we find the "calory" as a food unit referred to in popular speech and advertisements of foods. Professor Lusk suggests that it would be advisable to sell food by the 100 calories as a means of promoting economy,—by showing clearly how much actual food value we can get for our money if expended for one food rather than another.

The conclusions referred to amount practically to these: (1) in the ordinary American and European dietary there is too much protein food (such as meat and eggs); and (2) the needed reduction in protein can be almost instinctively accomplished through thorough mastication.

The first of these conclusions was originally put forward by my colleague, Professor Chittenden, Dean of the Sheffield Scientific School, whose work and standards form the starting point for this little book. Since Professor Chittenden first presented[3] his epoch-making low-protein idea it has won almost universal acceptance in theory although it required the shock of war to bring about a wide observance in practice.

The Great War has compelled radical and varied changes in diet, the significance of many of which we do not yet know. In some cases under-nutrition, or even actual starvation, has, of course, resulted. In other cases, if we may judge from such reports as have so far come to hand, the enforced restrictions on diet have actually benefited the people. There seems much in all this war-experience to confirm the conclusions of this book.

Following Europe, but as yet a long way behind, we in America are now, under Mr. Hoover's able leadership, called upon to economize in food.

One of the great economies is the partial substitution of vegetable for animal food. Jordan and

[3] "Physiological Economy in Nutrition," 1904.

others have shown that only from 3% to 18% of the food value is reclaimed by feeding animals and thereby transforming the foods eaten by them into flesh to be eaten by us. The remainder, 72% to 97%, is lost, although it must be remembered, of course, that much of this, such as garbage, is unfit for human consumption. The saving by direct consumption rather than feeding to animals is most significant in the case of grain.

If the conclusions here stated are correct, this and the other economies required of us in America, instead of threatening to impair our national vitality, promise to increase it materially; for what we are asked to do amounts to giving up those food practices, such as the excessive use of meat, white bread and sugar, which practices, though very common among us, are condemned by physiologists.

It gives the author some satisfaction to feel that this little book, written long before the war and without any other thought than promoting sound ideas of healthful living, may do its bit toward the war-economies now so sorely needed.

Needless to say, the book is not intended to cover the whole subject of diet, even from the point of view of a layman. It has little to say of the importance of using some hard, some bulky, and some raw foods, of free water drinking, of avoiding overweight and of frequent and thorough elimination of the food wastes from the alimentary tract.

It aims merely to establish, so far as one experiment may, the surprising increase in muscular endurance to be gained through a reduction of protein to the Chittenden standard and to show how easily and naturally this may be reached by simply abandoning the vicious American habit of hurrying through our meals.

IRVING FISHER.

New Haven, August, 1918.

THE EFFECT OF DIET ON ENDURANCE

INTRODUCTION

THERE appears to be very little literature on the subject of endurance. Since the epoch-making work of Mosso, much has been written on fatigue, and many varieties of ergographs have been constructed to record muscular fatigue; but no systematic study of endurance as such appears to have been made. Even the concept of endurance, as related to strength and fatigue, has been lacking. No correlations have been worked out between endurance and the factors upon which it depends.

The results of the experiment demonstrated so great an increase of endurance as to seem at first incredible. It certainly was a surprise, both to the men and to me. But statistics which I have been collecting have prepared me to find great differences and changes in endurance. The special result of the present experiment is to show that diet is an important factor in producing such alterations. The fact that endurance, even among persons free from disease, is one of the most variable of human faculties—far more variable than strength, for instance—is evident to any one who has made even a superficial examination. Some persons are tired by

climbing a flight of stairs, whereas the Swiss guides, throughout the summer season, day after day spend the entire time in climbing the Matterhorn and other peaks; some persons are "winded" by running a block for a street car, whereas a Chinese coolie will run for hours on end; in mental work, some persons are unable to apply themselves more than an hour at a time, whereas others, like Humboldt, can work almost continuously through eighteen hours of the day. Among statistics gathered independently of the present experiment, I have found measurable differences between persons far greater than any change of endurance of the subject of the present experiment.[4] Among some 50 tests of different persons holding their arms horizontally, many were found whose arms actually dropped against their will inside of ten minutes, whereas several were able to hold them up over an hour, and one man held them 3 hours and 20 minutes, or a round 200 minutes, and then dropped them voluntarily. Similarly with deep knee-bending, some persons were found physically unable to rise again from the stooping posture after accomplishing less than 500 bendings, whereas several succeeded in stooping 1000 times, and in one case, 2400. Again in leg-raising, the legs positively refused to rise to the vertical in some cases before 40 times were reached,

[4] For an account of some of these statistics, see "The influence of flesh-eating on endurance," *Yale Medical Journal*, March, 1907.

whereas in two cases this motion was performed 1,000 times or over. On the new ergograph mentioned below, among the 16 preliminary tests there was a range in endurance between different persons from 18 to 1000, and in the same person at different times from 29 to 110.

It is, to say the least, remarkable that hitherto so little effort has been directed toward discovering the factors which explain such differences in endurance. That exercise is one of the most and perhaps the most important factor has alone been recognized. A correspondent assures me that by means of moderate *regular* exercise he succeeded in increasing his endurance between 100 and 200% in three weeks as measured by leg-raising and "dipping." The influence of diet has always been regarded as small or negligible, and the opinion has been almost universal, until recently, that a diet rich in protein promotes endurance. Even among those whose researches have led them to the opposite conclusion, there is very little conception of the extent to which diet is correlated with endurance. Such a person, a medical friend of the writer, stated, when the present experiment was planned, that he did not think the dietetic factor strong enough compared with others to produce any marked effect. We have all heard, of course, of the enthusiastic reports of vegetarians as to their increased endurance, but these we have discounted as exaggerations. The result of the present experiment, however, would

the food; liquid foods to be sipped and tasted, not drunk down like water. There should be no artificial holding of food in the mouth beyond the time of natural swallowing, even if, as is to be expected at the start, that swallowing is premature. It is not intended to "count the chews," or hold the food forcibly in the front of the mouth, or allow the tongue muscles to become fatigued by any unnatural effort or position, or in any other way to make eating a bore. On the contrary, every such effort distracts one from the natural enjoyment of food. Pavlov has shown that without such attention and enjoyment of the taste of food, the secretion of gastric juice is lessened. The point of involuntary swallowing is thus a variable point, gradually coming later and later as the practice of thorough mastication proceeds, until the result is reached that the food remains in the mouth without effort and becomes practically tasteless. Thus the food, so to speak, swallows itself, and the person eats without thought either of swallowing or of not swallowing it; swallowing is put into the same category of physiological functions as breathing, which ordinarily is involuntary.

2. *Following instinct:* Never to eat when not hungry, even if a meal (or more than one, for that matter) is skipped; and when a meal is taken, not to be guided by the quantity of food offered, or by past habit, or by any theories as to the amount of food needed. The natural taste or appetite is alone con-

sulted, and the subject selects, from the food available, only those kinds and amounts which are actually craved by the appetite. After practice, the appetite gradually becomes more definite and discriminating in its indications.

These two rules—thorough mastication and implicit obedience to appetite—were alone employed during the ten weeks which constituted the first half of the experiment.

The second half of the experiment lasted about nine weeks. The same two rules which were employed during the first half were continued during the second, but a third rule was added. This was the use of suggestion, as follows:

3. *When instinct is in doubt, use reason.* This rule consists of acquiring and applying a little knowledge of foods and food elements. For this purpose, in the present experiment, two lists of food were given. One was arranged in a tentative order of merit, beginning with fruits and ending with alcohol, and the other in the order of the proportion of protein. The men were then asked, when and only when the appetite was *entirely willing,* to choose the better foods and the low-protein foods in preference to those high in protein. In this way the men gradually shifted their diet upward in the two lists, and thereby pursued a little faster the same direction in which they had already been found to be unconsciously moving under the

influence of thorough mastication and implicit obedience to appetite.

It would too greatly lengthen this report if any attempt were made to repeat in detail all the specific advice given to the experimenters under Rule 3. What has been said covers in a general way all the points except the advice (subject always to the consent of appetite) to eat light and quickly digested suppers.

Careful record of the amounts of food eaten and the constituents of protein, fat and carbohydrate was kept for each man each day, certain days being omitted if for any reason the record was incomplete, as when, for instance, the men were out of town or took their meals away from the club. To avoid weighing at the table, the food was all weighed in the kitchen and served in "standard portions" of 100 calories each, or simple fractions or multiples thereof, and the men merely recorded the number of portions eaten. The proportions of protein, fat and carbohydrate were found by means of the writer's "Mechanical Diet Indicator."[5] Atwater and Bryant's tables were used as a basis for calculation. For the first few weeks the figures were probably subject to some errors, and in all cases more or less guessing had to be practiced

[5] For a description of this instrument, see the writer's "A New Method of Indicating Food Values," *American Journal of Physiology*, April, 1906. For a description of its practical uses, see "A Graphic Method in Practical Dietetics," *Jour. of the Amer. Med. Assoc.*, Apr. 20, 1907.

with reference to the amount of lean and fat of meats; but the influence of any errors on the results must necessarily be small, because meat supplied, at the highest, only a small fraction of the total calories. It is believed that the results are, in general, correct to two significant figures.

For the first two weeks of the first half of the experiment, the men ate in their ordinary way. During the following eight weeks they masticated more thoroughly and followed the leadings of taste more carefully. Most persons, while nominally following taste, are largely controlled in their selection of foods by many other circumstances,—as, *conventionality,* or the desire to eat what others eat and the unwillingness to appear "different"; *politeness,* the desire to please one's host and hostess; *food notions,* the opinion that certain foods and certain amounts of food are "wholesome" even if not palatable and that certain foods should be avoided as injurious even if delicious to the taste; *narrowness of choice,* as at a boarding house table, which often supplies what is not wanted and withholds what is; and *habit,* by which the particular kinds and amounts of foods which have become customary through the previous causes—conventionality, politeness, food notions, and narrowness of choice—are repeated day after day without thought. The subjects of the present study were given a wide range of choice, the menu including fruits, nuts, cereals,

puddings and pastry, vegetables, milk, meats, etc. Meat if desired was available three times a day.

The object of the experiment was to find what effects on diet and endurance would follow from a strict obedience to the taste-instinct, when this instinct was given a longer chance to act by prolonged mastication and attentive tasting. Each man was therefore encouraged to choose his own food out of the menu for the day. Nothing was set before him until it was ordered, and even after a food was ordered it was not eaten if taste did not so dictate. The men were specially warned, during the first half of the experiment, against any conscious effort to decrease their food, protein, or meat; and while it is possible that subconscious suggestion played a part, so far as could be observed they were freer from its influence than any ordinary experimenter who might take up the same experiment after reading Mr. Fletcher's or Professor Chittenden's books.

That this conclusion as to the relative absence of subconscious suggestion is correct was evidenced by the experiences both before and after this part of the experiment. For a month prior to its actual beginning (Jan. 14), the experiment had been fully decided upon, and its plan and scope understood by the men. Had subconscious suggestion played an important rôle, it would probably have shown itself in a reduction of protein during this month; but determinations of the grams of nitrogen daily excreted in the urine,

taken at the beginning and end of this month, indicated no substantial change, as the following table shows. (M. does not appear in this table, owing to the absence of any specimen for December.)

TABLE I.

B	E	Lq	Lw	P	R	T	W	Average
Middle December								
11.2	11.1	13.8	12.3	11.3	13.9	14.2	15.9	13
Middle January								
10.4	12.7	14.3	14.3	11.1	14.8	12.2	15.4	13.1

On the other hand, during the second half of the experiment (Mar. 28-June 1), when the force of suggestion was consciously introduced, the reduction of flesh and protein went on rapidly, as is seen in Table II.

The facts, therefore, seem to show that the men followed directions closely, avoiding largely the influence of subconscious suggestion and following that of conscious suggestion in exact accordance with the directions given them.

CHANGES IN DIET

DURING the first two weeks of the first period when no change of habits was undertaken, the food showed little tendency to change in amount or in kinds. On the other hand, for the remaining eight weeks, during which thorough mastication and instinctive eating

were practiced, there was a distinct though gradual tendency toward reduction in the amount of food, in the quantity of protein, in the quantity of flesh foods. Exact figures were kept for calories, protein and flesh foods. These showed that the total calories gradually fell about 10%, the proteins, 15%, and the flesh foods, 40%.

In the second period, during which the force of suggestion to reduce protein and flesh foods was added, the same effects were noted in a still greater degree. During this period the calories dropped nearly 20%, the protein over 25%, and the flesh foods about 70%. By comparing the diet at the close of the entire five months of the experiment with the diet at its beginning, it was found that the total calories had fallen about 25%, the protein about 40%, and the flesh foods over 80%, or to about one-sixth of their original amount. A part of the reduction, at least of the calories, is probably due to the change in season, as the experiment began in cold weather and closed in hot weather.

These results are shown in the following table:

TABLE II.

AVERAGE DIETETIC RECORDS OF ENTIRE CLUB

	Week	Average weight	Average no. of 'portions'[6] daily	Calories of protein per lb. of body wt.[7]	Daily 'portions'[6] of flesh foods
1st Period	Jan. 17–23	149.8	28.3	2.7	2.4
	24–30		30.3	2.6	2.1
	31–Feb. 6		27.8	2.5	1.6
	Feb. 7–13		27.6	2.4	1.2
	14–20		25.8	2.1	.9
	21–27		26.4	2.2	1.1
	28–Mar. 6		25.3	2.2	1.2
	Mar. 7–13		24.6	2.1	1.3
	14–20		25.9	2.2	1.1
	21–27	148	26.7	2.2	1.4
2d Period	28–Apr. 3		26.7	2.1	.8
	Apr. 4–10		25.7	1.9	.5
	11–17[8]		27.3	1.7	.4
	20–26[9]		26.1	1.7	.1
	27–May 3		25.5	1.9	.5
	May 4–10		25.7	1.9	.4
	11–17		26.2	1.9	.4
	18–24		24.9	1.7	.4
	25–31		22.2	1.4	.4
	June	144			

Remembering that a "portion" is 100 calories, we see that, during the first four weeks, the men consumed

[6] Each "portion" being 100 calories.

[7] This column is calculated throughout on the basis of the body-weights on Jan. 14.

[8] Except E., M. and P.

[9] Except E. The last two days of the Easter recess, Apr. 18, 19, are omitted in this table.

an average of from 2760 to 3030 calories per day, of which 120 to 240 were in the flesh foods, such as meats, poultry, fish and shellfish, and that 2.4 to 2.7 calories of protein were ingested for each pound of body-weight. Translating Professor Chittenden's figures for the physiological requirement of ingested protein, we find it to be from 1.3 to 1.7 calories per pound of body-weight. Thus the men were at this time consuming nearly double the Chittenden allowance. During the last four weeks of the experiment all these magnitudes were lower. The per capita calories ranged from 2220 to 2620, of which only 40 were in flesh foods, and the protein had fallen to 1.4 to 1.9 calories per pound of body-weight, which corresponds closely to the Chittenden standard.

Although the protein used at the beginning of the experiment varied greatly among the nine men, at the end of the experiment it was reduced to a fairly uniform level for all the men. Moreover, the protein at the end corresponds closely with the results of Professor Chittenden's experiments. This seems especially significant in view of the fact that this level was reached unconsciously—for only one of the men, Lq., who used the mechanical diet indicator for the entire club, knew regularly the exact character of each man's food proportions—and without any food prescription such as was employed in the experiments of Professor Chittenden. This may mean that there is a simple way of reducing protein to the level of "physiological

economy," open to the ordinary man, without the necessity of special knowledge of foods and without the necessity of weighing and measuring food, either by the subject himself or by others.

Aside from the changes in protein, the proportions of food elements did not change greatly through the period of the experiment, the percentages of fat and carbohydrate in the total fuel value remaining very nearly constant. At the close of the experiment it was found that, for all of the men, the protein in proportion to the total fuel value was very nearly 10%, having been reduced from an average of about 14%. This reduction in the percentage of protein was almost entirely offset by the increase in the percentage of fat, which rose from about 30% to about 33% on the average. The percentage of carbohydrate thus remained almost constant. Individual variations were much less than might have been expected. The protein at the close of the experiment among the different subjects deviated very little from 10%; the proportion of fat varied from 28 to 36%, and the carbohydrate from 51 to 62%.

The results of the experiment may throw some light on the problem of the proper amount of food and food constituents for healthy men eating in a natural manner. For the five men, Lq., Lw., M., R. and W., whose weights showed the least tendency to fall and whose average weight at the close of the experiment was 151.4, we find the average total calories were 2620,

of which 10.7% was protein, 33% fat, and 56.3% carbohydrate. The number of calories agrees closely with the estimates (for sedentary persons) of Atwater and Benedict by means of the calorimeter.

During the first period all except E. and Lw. reduced their consumption of flesh foods considerably. It is noteworthy, as Tables IV-VI will show, that these two were the men whose improvements in endurance were probably among the least during this period.

During the second period Lq., W., P. and B. virtually abandoned flesh foods entirely, the "portions" consumed daily averaging nearer zero than .1. These men improved greatly in endurance also. On the other hand, E., Lw., M. and R. reduced their flesh foods the least, and their ranking in respect to increased endurance was in general relatively low.

EXCRETIONS; STRENGTH

THE following is a table of nitrogen excreted in the urine. It will be seen that the reduction in nitrogen daily excreted corresponds in general to the reduction in protein consumed.

TABLE III.

GRAMS OF NITROGEN EXCRETED DAILY[10]

B	E	Lq	Lw	M	P	R	T	W
Middle January								
10.4	12.7	14.3	14.3	8.7[11]	11.1	14.8	12.2	15.4
First April								
6.6	14.7	9.2	11.1	....	6.3	11.6	12.4	9.0
Middle June								
6.3	13.1	8.4	....	13.7	6.1	....	8.8	9.4
N. in middle June per kilog. of body-weight								
.093	.22	.12	....	.21	.09	....	.12	.13

This table shows that all the men excepting E. and M. greatly reduced their nitrogen excretion during the experiment, and that at the close (with the two exceptions noted) the men were on about the same nitrogen level as the subjects of Professor Chittenden's experiment, namely, near one-tenth of a gram of nitrogen per kilogram of body-weight.

Through the kindness of Francis G. Benedict, now director of the Carnegie Nutrition Laboratory, nitrogen analyses were made in December, 1906, six months after the close of the experiment, to discover to what extent the men had adhered to their newly acquired diet after the eating club in which it had been practiced was disbanded. The results were B. 11.0,

[10] Each figure is obtained by averaging 2 or 3 consecutive days' specimens.

[11] Jan. 23 and Feb. 10.

Lq. 10.5, Lw. 7.9, M. 9.9, P. 6.8, R. 11.5, T. 11.9, W. 8.9. These show that half of the men had reverted to some extent toward their original diets. The men state that the reason for this reversion was the difficulty in selecting food differing greatly in kind and amount from that customarily served at their boarding houses.

Gymnasium tests were made at the beginning, middle and end of the experiment. These tests were of two kinds,—tests of strength and tests of endurance. The times of the tests were widely separated, partly because those of endurance were too exhausting to be often repeated, and partly because it was desired to avoid the influence of "practice"; for not only does practice increase strength and endurance, but it also gives the users of the strength-registering apparatus a facility or "knack" in manipulating it which produces a false appearance of improvement.

The dates of the three tests were January 14, a week after the end of the Christmas vacation; March 28, just before the Easter recess; and June 16,[12] just before the summer vacation.

During the first period there was a slight increase in strength (from an average "total" strength of 1076 to 1118), and during the second period a slight fall to 995, which is about 12% from the mid-year's 1118, and about 8% from the original 1076. Thus the

[12] But May 31 for E., Lw., R. and W., on account of earlier examinations than the others, necessity to leave town, etc.

strength of the men remained nearly stationary throughout the experiment.

CHANGES IN PHYSICAL ENDURANCE

It is fortunate that the strength of the men remained so nearly stationary; for it demonstrates the more clearly that the increase in endurance which will be shown below was an increase in endurance *per se*, and not in any degree due to an increase in strength. Strength and endurance are entirely distinct and should be separately measured. The strength of a muscle is measured by the utmost force which it can exert *once;* its endurance, by the number of times it can repeat a given exertion *well within its strength.*

After much consideration and consultation it was decided not to place reliance on the ordinary ergographs as a means of measuring endurance.[18] Instead

[18] The reasons, in brief, were (1) because these ergographs are adapted to testing only a few unimportant, and for the most part unused, muscles; (2) because, in operating these devices, the subjects do not simulate real work, since the muscles are placed in an awkward and unnatural position in which "no purchase" is felt; (3) because experience has shown that subjects waste their effort by expending it not only while raising but while lowering the weight, and that this waste during the period of relaxation varies greatly with different subjects; (4) because a fixed weight is used instead of a weight proportionate to the different strengths of the various subjects. One might as well attempt to test the walking powers of a woman weighing 100 lbs., as compared with those of a man weighing 200 lbs., by compelling the woman to carry a 100-lb. weight

simple gymnastic tests of physical endurance were employed:

(1) Rising on the toes as many times as possible.

(2) Deep knee-bending, or squatting as far as possible and rising to the standing posture, repeating as often as possible.

(3) While lying on the back, raising the legs from the floor to a vertical position and lowering them again, repeating to the point of physical exhaustion.

(4) Raising a 5-lb. dumb-bell (with the triceps) in each hand from the shoulder up to the highest point above the head, repeating to the point of physical exhaustion.

(5) Holding the arms from the sides horizontally for as long a time as possible.

(6) Raising a dumb-bell (with the biceps) in one hand from a position in which the arm hangs down, up to the shoulder and lowering it again, repeating the motion to the point of physical exhaustion. This test was taken with a 50-lb. and then with a 25-lb. dumb-

so that she might walk with the same weight as the man. Some of these objections have been met in special instruments, such as that of Prof. W. S. Hall of Northwestern University.

After the experiment was half over, and too late to make use of it, the writer devised an ergograph which, it is believed, meets all of the above objections. He was led to do so by the fact that the tests employed were so frightfully exhausting to the men.

bell. (The records of the test with the 50-lb. dumb-bell are omitted in Table IV, being considered separately below.)

The following table (IV) shows the results of the three sets of tests in January, March and June.[14]

[14] The order in which the tests were taken was not the same for all of the nine men, owing to the lack of a sufficient number of gymnasium assistants in taking the tests. But care was taken that each man should himself preserve the same order in all three series of tests. Thus, for the March series he took the first two or three tests which he had taken in January. This explains why, in the March series, the tests as shown in the tables are not the same for all the men. The order of the January and June tests for the different men is given below. The tests which were taken in March are in italics.

B	*1*	*2*	3	4	5
E	*1*	*2*	3	4	5
Lq	*1*	*2*	*4*	3	5
Lw	*1*	*2*	*3*	4	5
M	*1*	*5*	*2*	3	4
P	*3*	1	5	2	4
R	*1*	*2*	3	4	5
T	*1*	*2*	3	4	5
W	*3*	*5*	1	2	4

TABLE IV.

TESTS OF PHYSICAL ENDURANCE

			B	E	Lq	Lw	M	P	R	T	W
(1)	Rising on Toes	Jan.	300	1007	333[15]	69[15]	127	1482	702	900	1263
		Mar.	400[16]	1265[16]	2620[18]	65[15]	400[21]	....	831[16]	1500[18]	....
		June	500[16]	1061[21]	3000[19]	85[15]	1500[18]	1800[16]	1263[16]	1800[17]	3350[19]
(2)	Deep Knee Bending	Jan.	82	142	70	48	132	208	374	129	404
		Mar.	....	....	191[21]	47[21]	....	....	....	....	....
		June	200[16]	81[21]	202[16]	58[21]	155[16]	230[16]	453[21]	250[18]	508[18]
(3)	Leg Raising	Jan.	25[20]	52[20]	9[17]	22[20]	30[20]	27[20]	50[20]	23[20]	30[20]
		Mar.	....	....	....	33[20]	....	34[20]	....	....	40[20]
		June	33[20]	38[20]	20[20]	35[20]	31[20]	37[20]	103[20]	19[20]	53[20]
(4)	5-lb. Dumb-bell (triceps)	Jan.	75[20]	138[20]	78[20]	38[20]	51[20]	44[20]	100[20]	83[20]	185[20]
		Mar.	....	....	106[20]	....	....	....	....	....	....
		June	127[20]	59[20]	80[20]	51[20]	75[20]	56[20]	104[20]	101[20]	501[20]
			M S	M S	M S	M S	M S	M S	M S	M S	M S
(5)	Holding Arms Horizontal	Jan.	5– 0	1–33	4– 7	3–37	3–30	5–39	2– 5	3–22	11– 0
		Mar.	....	....	....	....	5–49	....	....	....	15–35
		June	9–36[20]	2–56[21]	3–50[21]	3– 0[21]	6– 5	10– 1[16]	3–16[21]	3–24[21]	23–45[21]
(6)	25-lb. Dumb-bell (biceps)	Jan.	50[20]	18[20]	16[20]	6[20]	20[20]	11[20]	10[20]	25[20]	54[20]
		June	105[20]	10[20]	26[20]	33[20]	30[20]	29[20]	27[17]	75[17]	108[17]

[15] Cramped.
[16] Not as tired as in January test.
[17] Not to limit.
[18] Not nearly as tired as in January test.
[19] Not tired.
[20] To limit of muscle's capacity.
[21] About same fatigue as in January.

This table will repay careful study. From it we see that with one exception (E.) all of the men had improved in the March and June tests as compared with the January tests, and the eight men who did improve showed improvement in every test, except Lq., Lw. and T., who showed slight falling off in individual cases.

As inspection will show, some of the increases are remarkable. The recorded increases in the 60-odd cases were, with a few exceptions noted below, all true increases and not due to increased effort to break a previous record. In anticipation of such possible effect of ambition, the men were urged in the January tests to the utmost limit they could or would stand. The original intention had been to work each muscle tested until it was physically unable to repeat the motion, but this was not usually found practicable, except in tests 3, 4 and 6, and in some cases 2. In the other tests the will gave out before the muscles. The March and June tests were so managed that when a man had surpassed his January record he was not allowed to proceed beyond the degree of fatigue which he had reached in the first test. This was usually not a difficult matter, as the fatigue in January had been difficult matter, as the fatigue in January had been excessive and the men had no desire to suffer again the painful after-effects. Hence, with the exceptions to be noted, the March and June records not only exceeded those of January, but were accomplished with much less fatigue. The actual improvement was therefore greater than the recorded improvement.

CRITICISM OF RECORDS OF PHYSICAL ENDURANCE

THAT the fatigue after the March and June tests was in general much less than after the January test was made evident by three substantial proofs. The first was the feelings of the men themselves as recorded in the footnotes to Table IV. After the March and June tests, every man of the eight who showed improvement felt "not tired," or "less tired than in January tests," which is the same as saying "not exhausted"; or else he had gone "to limit" as in January, which means that the muscle itself was unable to repeat the exertion required. The last was usually true of the "leg-raising," "raising 5-lb. dumb-bell (triceps)" and "raising 25-lb. dumb-bell (biceps)." The only tests in which there was the possibility of being mistaken as to the degree of fatigue were the "rising on toes" and "holding arms horizontal." In the former fatigue comes so slowly, and in the latter the pain is so intense that they prove to be tests of will power or "grit" quite as much as of muscle power. In these cases the men had some difficulty in remembering the original degree of fatigue. But the increases were so great and the men were so positive as to their feelings that there remains little room to doubt the substantial correctness of the results. In a few other individual cases, as of Lw. and Lq., whose records in test 1 were sometimes stopped by cramps, there is some room for

doubt as to the correctness of the recorded improvement.

The second proof that the fatigue of the men in the June tests was less than that in the January tests was found in the fact that the stiffness and soreness which followed in June were markedly less than in January and of much shorter duration. This was true of all the eight men who showed improvement, except R.

The third proof of less fatigue in June than in January for the eight men is that in June the men finished the ordeal of the endurance tests with more strength left than in January, although they *began* the two tests with slightly less strength. The fact that they had more strength left after the June test is made evident by the first part of test 6, given below, which in each case came after the endurance tests were nearly or quite finished. This consisted in lifting a 50-lb. dumbbell. The weight being so great, this was practically a test of strength rather than of endurance. Now all of the eight men who showed improvement in the *endurance* tests of Table IV, showed improvement in this *strength* test also, as the following table shows:

TABLE V.

LIFTING (by biceps) 50-lb. DUMB-BELL[22]

	B	E	Lq	Lw	M	P	R	T	W
January	0[24]	1[24]	1[24]	0[24]	1[24]	0[24]	4[24]	2[24]	13[24]
June	1[24]	0[24]	5[24]	8[24]	13[24]	1[24]	10[24]	12[23]	26[24]

But, as has been said, the strength tests taken *before* the endurance tests showed a slight falling off in June as compared with January for all but one (Lq.) of these eight men. In other words, in June the men *began* their endurance tests weaker than in January, but *finished* them stronger. The larger residuum of strength left after the June tests as compared with the January tests indicates that the June tests, in spite of being far more severe, fatigued the men less.

The 50-lb. dumb-bell test resolved the last doubts in my own mind whether, for some of the men, the recorded results might not exaggerate the true improvement. The two men of whose records I should have felt a little doubt were B. and R. Both of them came to the June test after prolonged mental exertion, and their exhaustion at the end was far more evident

[22] This part of test 6, being one of strength rather than of endurance, was not included in the endurance Table IV. Had it been included it would have increased even more the percentage of improvement shown, for it shows an average increase from 2.4 to 8.4, or 250%.

[23] Not to limit.

[24] To limit of muscle's capacity.

than that of any of the others. That it was great is clear from their own statements given below, though only R. reported himself as having been about as stiff and sore after the June as after the January tests. But both B. and R., whereas they had less strength before the June endurance tests than before the January tests, had more strength left (Table V) after the June tests than after the January tests. At the close of the January tests they were so exhausted that B. could not raise the 50-lb. dumb-bell at all and R. could raise it only 4 times. Had their exhaustion after the June tests been as great, it seems certain that B. would still have been unable to raise it, and R. would have been unable to raise it more than 4 times; but as it was, B. raised it once and R. 10 times.

The value of such a positive proof that the June tests were more easily endured than those of January was not perceived until the figures were analyzed. Had it occurred to me in time, all the strength tests taken before the endurance tests would have been repeated after them. It is true that the strength tests at the beginning were not of the same muscles as those (the biceps) used in the strength test by dumb-bells at the end, but the strengths of different muscles for the most part vary in unison with each other.[25]

It is significant that the only man whose strength,

[25] Out of the 108 comparisons of strength (i.e., comparisons for each of nine men in each of four tests for January vs. March, March vs. June, and January vs. June), only 20

as shown by the above table, was less at the close of the June experiment than at the close of the January experiment was E., who was also the only man whose endurance showed any reduction. The facts, therefore, in his case are not discordant with those already stated; for E. was the least assiduous in conforming to the conditions of the experiment. This was often remarked, both to him and to me, by the other members of the club, and it was suggested more than once that I should "nudge" him. But, as I had been desiring a "control," or a subject in which all the conditions except mastication were the same as for the other men, I decided to say nothing. The result was instructive, for E.'s case stood out as exceptional in almost all respects. His reduction in quantity of food, except for a spurt at the end, was less than of most of the men; his reduction in protein, with the same exception, was the least of all; his reduction in quantity of flesh foods was the least of all; his nitrogen in June (Table III) was one of the highest; his reduction in volume of urine was one of the lowest two; his improvement in the fecal tests was third to lowest; his loss of strength was second greatest; and as to endurance, he was the only one who failed to show improvement.

are discordant with the general trends as shown by the totals. Thus, for B. the general trend between March and June as shown by the total was downward, and this downward trend is found in all but one of his four tests, the discordant case being the "back lift."

PERSONAL IMPRESSIONS AT END OF EXPERIMENT

THE men kept diaries in which are recorded their sufferings after the various tests. These show a decided lessening in stiffness and soreness in the later tests, though in the June tests the men had generally done double the amount of work that they had done in January. It would have been a physical impossibility to do as much in January as was easily accomplished in June in tests 3, 4 and 6; and granted that it had been even possible in January to goad the men to do as much in tests 1, 2 and 5 as they did without urging in June, they must certainly have been ill.

As to illness, in the course of the experiment there were the usual winter colds, though apparently these were less common than before. One man had grip for a few days, another the mumps, and several had constipation. In general, the men expressed themselves as better than usual and in some cases they were very enthusiastic.

The following extract from the diary of B. is, I think, typical of the facts in this respect to general health: "Have now, March 23, slight sore throat. In regard to colds, I have been troubled less this year than at any time for years; but this fact may be due to great change in climate, Nebraska to Connecticut. Have usually had colds more or less all winter; therefore my freedom has been indeed remarkable. From

September last till the beginning of the experiment I experienced frequent attacks of indigestion, 'heartburn.' Have been almost free from that, though two or three times I had the same experience after eating bananas."

The following are further statements from the men:

(B.) I was *very sore* [after the June test, Saturday, June 16, 1906]. However, I think the soreness was not so severe or lasting as it was after the January test. After the earlier test the calf muscles were hard and knotted for several days; but this time, while they were sore, they were almost normally soft. Saturday evening when I went to bed they were quite hard, but Sunday morning they were normal and practically remained so.

I have no doubt that in my case there was great increase in endurance, though I think that I lost in amount of energy that I could exert at any given moment. This loss is due perhaps to two things: (1) I took, on the whole, less exercise than during the time preceding the January test; (2) I had been working quite hard for three months steadily, while the January test followed a three weeks' vacation during which I did little or nothing. As to increase of endurance there can be no doubt. For example, in the deep knee-bending, I began to get tired at 50 and had no idea of going above 100. When I reached this I set my goal at 125, then 150, 160, and was able to reach 200 before I was exhausted. In January, after I was

tired I was not able to go on very long before I became completely exhausted. This shows increased endurance. I had the same experience in the other hard physical tests. Considering everything, I have no doubt that I was able to hang on much longer, after I began to get tired, than in January.

I am at a loss to ascribe the increased endurance to anything else than to the diet. My way of living otherwise continued about the same after the January test as it was before. Personally I am convinced that the increased endurance must be due to diet and manner of eating; all other factors that I can think of are unfavorable rather than favorable to more endurance.

(E.) All effects of [June] test disappeared entirely within four days. [Effects of January test lasted six days.]

(Lq.) The stiffness and soreness had entirely disappeared in four days. They were not nearly so severe as in the test in January. After I was through in January I could hardly go down the stairs of the Gymnasium, and three days after the test going up and down stairs was accompanied with a great deal of pain. I was stupid mentally for a whole week the first time, but in the last test I passed that stage in a couple of days.

(Lw.) After the tests in January I could not straighten my arm, but I could after the last test [in June] in spite of the extreme soreness.

(M.) The stiffness and soreness were entirely

worn off in two days. I did not feel it nearly as much as I did last January. In fact, I did not exert myself to the utmost this last time because I had several examinations to take a day or two later.

My experience has shown me that I was at my best in mind and body when I ate meat four times a week. I have tried both more and less and found the above to be the medium. I also found that I could do more when I had the largest meal at noon.

(P.) I went into the second test with some trepidation, knowing that I had lost considerable weight the preceding ten weeks. Physically, I cannot say that I felt stronger before the second test than before the first; nor did I feel weaker. As the test developed, however, I soon saw that my endurance had increased.

Generally speaking, the soreness was less extensive, less trying or acute, and (I think) shorter-lived than in January.

I can ascribe gain in endurance to nothing but the diet and thorough mastication. Every other factor in the situation was against this gain—exercise, of which I took certainly no more than usual and in the latter weeks much less; work, of which I had had a long, hard pull as against the three weeks' rest preceding the January test; sleep, much decreased for most of May and June. You stated last December that you wished every factor to be *in favor* of the first test and *against* the second. This condition has been true in high degree for my case.

(R.) The outcome of the last endurance test was about the same as the one held January 14, 1906. I was fearfully sore for about one week, reaching the climax at the middle of the week.

(T.) Throughout the test I passed from one event to another with much shorter periods between than I did in January. With the exception of lying on my back and raising my feet, I at no time approached as near exhaustion as I did in January. In January, in rising on my toes and in the deep knee-bending, I continued till I fell to the floor. I was not exhausted at the close [of the June test], but marked papers for 2½ hours before going to bed.

(W.) May 31. After the test I felt fairly tired and ready to quit—however, not nearly so exhausted as before in January.

June 1. Sore in thighs and biceps, also felt my abdominal muscles.

June 2. Expected to be much worse on this the second day, as in January, but not so. About same as yesterday. Later in the day could run upstairs two steps at a time as I could yesterday—a thing undreamed-of in January for over a week after the test.

ESTIMATED IMPROVEMENT IN ENDURANCE

THE following table expresses the percentage of improvement in the records of Table IV.

TABLE VI.

IMPROVEMENT IN PHYSICAL ENDURANCE IN PERCENTAGES[26]

		B	E	Lq	Lw	M	P	R	T	W
(1)	Jan.–Mar.	33+	26±	686−	−5±	215±	...	18+	66+	...
	Jan.–June	66+	5±	800−	23±	1081±	21+	79+	100+	165+
(2)	Jan.–Mar.	...	...	172±	−2±	...	...	...	...	...
	Jan.–June	144+	−43±	188+	21±	17+	10+	21±	94+	26+
(3)	Jan.–Mar.	...	...	...	50	...	26	...	...	33
	Jan.–June	32	−27	122−	59	3	37	106	−17	77
(4)	Jan.–Mar.	...	...	36	...	...	...	...	...	...
	Jan.–June	69	−57	2	34	47	27	4	22	170
(5)	Jan.–Mar.	...	...	...	...	66±	...	...	...	42±
	Jan.–June	92−	89±	−7±	−17±	74±	77+	56±	1±	115±
(6)	Jan.–June	110	−44	62	450	50	163	170	200	100+
Av.	Jan.–Mar.	33+	26±	298±	14±	140±	26	18+	66+	37±
	Jan.–June	85±	−13±	194±	95±	212±	56+	73±	66±	109±

[26] In Table VI most of the figures are succeeded by a "+", which signifies that the true improvement was greater than the

The table shows enormous differences in the figures even of the same man for the same period. Thus, the June improvement of W. reads 165 +, 26 +, 77, 170, 115 ±, 100 +. Such wide differences between the

figures indicate. Thus, the first entry in Table VI, "33+", means that B.'s improvement between January and March in test (1) (rising on toes) was *more* than 33%. Similarly, "686—" for Lq. in same test means that improvement was *less* than 686%. Again, "215±" for M.'s same test signifies that his improvement in this test may have been greater *or* less than 215%. Finally, when any figure is not followed by a sign, as for instance, B.'s (3) (leg-raising), the meaning is that the figure given is, humanly speaking, correct. This accuracy applies only to those tests in which the muscles were worked till they were physically unable to repeat the movement. The reasons for the various suffixes may be found by studying the footnotes of Table IV.

For instance, the "+" after 33 for B.'s (1) is explained by the fact (as indicated in the footnote to Table IV) that after his March test he was not as fatigued as after his January test, although he had improved upon his January record by 33%. The only cases in which the explanation of the suffixes will not be found from the footnotes to Table IV are the following: E.'s (1), 26±, in which case the "—" is inserted owing to the fact that E. had come to the March test after the refreshment of a nap; and M.'s (1), 1081±, in which case the "—" is inserted owing to the fact that this high figure is inconsistent with the other results of the test, it being thought that M. may have been mistaken in his remembrance of his January test. The "—" has been inserted whenever there was the slightest ground of any kind for thinking the figures might be overstatements. With these figures weeded out, the remaining ones certainly understate the actual improvement.

improvements in different tests seem puzzling at first, but they are explained, partly if not wholly, by two causes. The first is the obvious one that many of the figures are not exact records, but understatements, and naturally their margin within the truth will vary widely. Thus, the records for deep knee-bending (2) for W. show merely that the improvement is *over* 26%; the true figure may well be 100%, which would be more consistent with the other figures. But the deep knee-bending test had been found in January very painful and inconvenient in its after-effects, and there was therefore less inclination in the June tests to approach closely to the limit in this particular test.

The other reason is that in some tests a larger fraction of the total strength of the muscle tested was called into play than in others. Thus, "leg-raising" requires a very large fraction of the strength of the abdominal muscles, while "rising on toes" requires only a small fraction of the strength of the calf muscles. This may explain why, in general, the improvement in the test of the calf muscles seemed so much greater than in that of the abdominal muscles. This explanation is, however, purely hypothetical. It would be interesting to find out experimentally how much improvement in endurance is shown by a muscle when the muscle is exerted in different degrees, say to 75%, 50% and 25% of its strength-capacity.

Bearing in mind these two possible reasons for the variations in the figures, and also the fact that there

must have been more or less actual differences in the improvement of different muscles, we need not be surprised at the disparities which the table shows.

If we omit the cases in which the records are at all doubtful (with suffix ±) or exaggerated (suffix —), we have left the following table for the eight men who showed improvement:

TABLE VII.

PERCENTAGE OF IMPROVEMENT (exact or understated) OF EIGHT MEN

		B	Lq	Lw	M	P	R	T	W
(1)	Jan.–Mar.	33+	...	...	...	...	18+	66+	...
	Jan.–June	66+	...	...	...	21+	79+	100+	165+.
(2)	Jan.–Mar.	...	...	...	...	...	...	...	...
	Jan.–June	144+	188+	...	17+	10+	...	94+	26+
(3)	Jan.–Mar.	...	...	50	...	26	...	...	33
	Jan.–June	32	...	59	3	37	106	−17	77
(4)	Jan.–Mar.	...	36	...	...	...	...	...	...
	Jan.–June	69	2	34	47	27	4	22	170
(5)	Jan.–Mar.	...	...	...	...	...	...	...	...
	Jan.–June	...	...	...	...	77+	...	...	...
(6)	Jan.–June	110	62	450	50	163	170	200	100+
Av.	Jan.–Mar.	33+	36	50	...	26	18+	66+	33
	Jan.–June	84+	84+	181	29+	56+	89+	80+	107+

The figures of Table VII show an undoubted increase in endurance, both for the first half and more especially for the whole period of the experiment.

But, for an accurate presentation, we may carry our criticism one stage further. The figures given hitherto represent a conglomerate sort of endurance, made up of endurance of different muscles subject to different degrees of strain. As pointed out before, the calf muscles were called upon for only a small fraction of their strength-capacity, whereas the abdominal muscles were called upon for a very large fraction. Moreover, the fraction must have varied somewhat in different tests, according to the variation in strength and weight. An ideal test would be one in which the same fraction of strength was used.[27]

Fortunately, such an exact test is afforded by the 25-lb. dumb-bell. It followed immediately after the 50-lb. dumb-bell had been raised until the biceps was unable to repeat the motion. At the moment the 50-lb. test ended, the 25-lb. test began. At this moment the strength of the biceps was just at or barely below the fifty lbs. required to raise the heavier dumb-bell. In other words, in raising the 25-lb. dumb-bell the muscle needed *just fifty per cent* of its strength at the time the test began. The use of the 25-lb. dumb-bell gradually reduced this strength from 50 to 25 lbs. The test was therefore perfectly uniform for all the men;

[27] It is on this principle that the new ergograph, before referred to, is constructed.

it showed how many contractions were necessary in each case to bring down the strength of the biceps from 50 to 25; it showed how much the muscle could endure before being robbed, by fatigue, of half its strength. Thus, at the beginning, the strength is 50 lbs.; after the first contraction, it is, say, 49; after the second, 48, etc. But the contractions continue until the strength sinks below 25 lbs. The loss of strength may be said to measure fatigue. The *slowness* of this loss may be said to measure endurance and is well indicated by the number of contractions necessary to tire a muscle from a strength of 50 lbs. to a strength of 25 lbs.

Four exceptions, however, need to be noted. Three men, B., Lw. and P., were unable in January to raise the 50-lb. dumb-bell at all (see Table V). For example, Lw. in January, unable to raise the 50-lb. dumb-bell, raised the 25-lb. dumb-bell 6 times. The strength of his biceps at that time was less than 50 lbs., say 40 lbs., in which case the lifting of the 25-lb. dumb-bell required not 50% only but 62½% of its strength. In June he *could* raise the 50-lb. dumb-bell and after his biceps muscle was exhausted to that level, he raised the lighter dumb-bell 33 times. Thus Lw. is credited with an improvement of 450% because his record for the 25-lb. dumb-bell increased from 6 to 33. But in June he started on his series with the 25-lb. dumb-bell having available only 50% of the full strength of his biceps, whereas in January he started

on his series with the 25-lb. dumb-bell having available 62½% of the full strength of his biceps. To compare a 50% test of June with a 62½% test in January gives a record of improvement which is not one of pure endurance, but which includes the element of increased strength. This is "endurance" in the crude sense in which we may say a man has more endurance for carrying trunks than a boy; but for a comparison of pure endurance, the boy should be given smaller trunks to handle than the man.

The fourth case is E., to whom the reverse reasoning applies. In June when he reached test 6, he was unable to raise the 50-lb. dumb-bell at all, though in January he had raised it once. Hence, while the 25-lb. dumb-bell was a 50% test in January, it was a more severe one in June, and the —44% which records his falling off does not represent a pure loss in endurance, but partly also a loss of strength.

Making the four omissions just mentioned, we may use the remaining records from the last line of Table VI, as a barometer of *pure* endurance.

We therefore have three methods of estimating the increase of endurance between January and June. These may be put together in the following table:

TABLE VIII.

PERCENTAGE OF INCREASE OF ENDURANCE, JANUARY TO JUNE, BY THREE METHODS

	B	E	Lq	Lw	M	P	R	T	W
Average 6 tests	85±	−13±	194±	95±	212±	56+	73±	66±	109±
Omitting doubtful cases "+"	84+	...	84+	181	29+	56+	89+	80+	107+
"Pure" endurance of biceps	...	...	62	...	50	...	170	200	100+

The first line of this table tells us the average of the recorded improvement in endurance shown for each man. But as each such average is made up from the figures of Table VI, some of which, as indicated in that table, are possibly too high, some doubt necessarily attaches to it, though practically the only real cases of doubt are Lq. and M. The average of these averages is 101% for the entire club, and is probably within the truth; for most of the individual figures which go to make up this result are understatements, not overstatements.

The second line shows the average improvement in tests in which there is no doubt that the figure is at least not too high, though it may be too low. The average of these is 89%, and is therefore certainly too low an estimate of the average improvement for the eight men who improved at all.

The third line shows the increase of *pure* endurance (that is, endurance considered apart from strength) for the five men for whom the figures were available. The average of these is 116%.

We are quite safe in saying therefore that the average improvement of the eight men who improved was 90%. As to the degree of retrogression of E., it is difficult to say, though it is believed that the figures exaggerate it. This is certainly true of the 25-lb. dumb-bell test, for reasons given. My own impression, and E.'s also, is that he actually gained in endurance from the dietetic experiment, but that his gain

was not enough to offset the loss occasioned by (1) the hard term's work, which, as in the case of the other men, was a decided handicap, and (2) the omission of his customary exercises. This must have been a greater handicap for him than for any of the other men, since he had been accustomed for six years to heavy gymnasium training. If this interpretation is correct, we may liken the experiment to nine men trying to swim against a current. The eight who exerted themselves the most succeeded in forging ahead; the one who tried the least drifted backward, though the effect of the swimming (dieting) was to propel him forward. Whether or not E. was actually propelled forward by diet must remain a matter of conjecture or inference; but that the other eight men gained is an established fact.

SUMMARY

THE phenomena observed during the experiment may be summarized as a slight reduction of total food consumed, a large reduction of the protein element, especially for flesh foods, a lessened excretion of nitrogen, a reduction in the odor, putrefaction, fermentation and quantity of the feces, a slight loss of weight, a slight loss of strength, an enormous increase of physical endurance. These phenomena varied somewhat with different individuals, the variations corresponding in general to the varying degree in which the men adhered to the rules of the experiment.

That we are correct in ascribing the results, especially in endurance, to dietetic causes alone, cannot reasonably be doubted when it is considered that no other factors of known significance were allowed to aid in this result. On the contrary, so far as the operation of other factors was concerned, these must have worked against rather than for the results achieved. Exercise was in no case indulged in to a greater extent than had previously been the custom, and in most cases it was less. The men were warned not to take up exercise beyond what they had been accustomed to before the experiment began, and if they varied their exercise at all, to lessen rather than increase it. They were very conscientious on this point, as on others,—so much so that some of them at first gave up exercising until they began to feel "logy." This overzeal was corrected; but in no case have I reason to think that the exercise taken was more, or more systematic, than previously. M. was probably the most systematic in taking exercise.

The men did not practice on the endurance tests between times. This was expressly forbidden, and the men were too trustworthy to admit of a doubt on this point. The tests themselves, needless to say, were too far apart to have given any chance for repetition to give "knack," and were too severe to count as beneficial exercise.

Nor were the men more regular in their hours of retiring or other habits. On the contrary, they were

rather more reckless in burning the midnight oil. It developed that, with their increased freedom from fatigue, they indulged more freely than ever their propensity to work in the lines of their respective ambitions. At first they felt justified in doing this, as it accorded with their instructions not to remove any handicaps to their chance of improving their endurance, but to increase rather than decrease such handicaps. But this liberty became license, and I was forced to remonstrate with the men for their late hours and overstudy, which tended to rob them of their surplus endurance almost as fast as it accrued. Long before the experiment was finished the men had given every appearance of improved working power, but I was not at all sure that they would have any of it left to show in the final test, because of their tendency to use it up in work. It should be stated that all except M. were graduate students, and almost all of them, in addition to their university work, were earning their own way.

The advance of warm weather must have tended, had not their diet counteracted it, to tire the men, if, at least, we may trust common impressions as to "spring lassitude."

Again, the conditions immediately preceding the March and June tests, as compared with those preceding the January test, were such as to give the advantage to the January test. The latter came soon after the Christmas holidays, when the men, as they them-

selves stated, felt refreshed and at their best, whereas the March test came just before the Easter recess, after a hard term's work, and the June tests came after a like period of hard work,—in some cases, as of B. and R., immediately after exhausting examinations.

Finally, the tests themselves were serious drains on vitality. Each required a period of from several days to two weeks for recuperation, and each robbed the men temporarily of several pounds of weight.

When, therefore, we observe the known handicaps, —the overstudy, the strain of the tests, the advance of warm weather, the fact that the first test came after rest and the other tests after work, and when we are unable to find any other cause than diet—such as exercise, regularity of bedtime or other habits—we are forced to conclude that the only causes which produced the endurance were dietetic.

Possibly some persons may be disposed to find a convenient escape from this conclusion by ascribing the improvement to suggestion. Under this theory, the men improved because they expected to. It is quite true that there may be more force in autosuggestion than most of us realize. But, fortunately, for the present case we scarcely need to argue the point; for as a matter of fact it was not true that all of the men expected to improve. This was certainly not true before the March test. In fact, the men were about equally divided in their predictions as to the outcome, and used to have animated discussions. Yet, both

the confident and the skeptic faction improved in endurance in the March test; and so far as I am acquainted with their prognostications and have noted their improvement, there was little if any correlation between those prognostications and their improvement.

It is of course still *possible* that some unobserved element has crept into the case, to which, and not to the diet, the improvement in endurance was due; but in view of all the facts recited, this is extremely *improbable*. What slight doubt remains should be resolved by further studies. I earnestly hope that other and more careful studies may be made by more competent investigators than I.

We conclude that the improvement in endurance was exclusively due to dietetic causes. The only dietetic causes at work were (1) thorough mastication, (2) implicit obedience to appetite, (3) (during the second half of the experiment) when appetite did not clearly determine the choice, the voluntary selection of the non-flesh and low-protein foods, and (4) an ample variety of good foods, well cooked.

If we allow ourselves to speculate as to the changes in the character of diet which were produced by thorough mastication, we may draw an inference from the fact that the carnivorous animals are fast-eaters, whereas the grain-eating animals are slow-eaters. It would seem, therefore, when man changes his habits from fast eating to slow eating he naturally changes

his food from the food of a fast-eating to that of a slow-eating animal. The question, therefore, which is the natural food for man, may possibly be associated with the question, which of the two methods of eating is natural to man. Was the slow eating of the nine men an artificial and unnatural practice, as would be indicated from the fact that the majority of men eat far faster? Or, are the ordinary habits of man in respect to the manner of fast eating themselves unnatural? I have not attempted to gather the facts necessary to solve this problem, but it certainly constitutes an interesting one for the physiologist and anthropologist. The few facts upon which I have chanced to fall would seem to indicate that man is naturally a slow eater, and that the hurry-habit to which most of us are prone is a consequence of the artificial high pressure to which modern civilization has subjected us. Certain it is that the conditions which give rise to quick-lunch counters and to the short stops of trains for refreshments, were produced, not in order to meet any natural propensity to eat fast, but on the contrary, in the interest of the more rapid transaction of business, with which mealtimes are regarded as an interference.

We may therefore at least conclude that whatever the speed of eating which is natural to the human animal, his actual speed under civilized conditions is greater than natural. It is noteworthy also that children are very deliberate in eating their cookies. It is

only after they are reproved for keeping their elders waiting that they begin to imitate the latter and bolt their food. Dr. Higgins[28] and Dr. Hasse[29] have pointed out also some physiological considerations, based on the anatomy of the human throat compared with the throats of the carnivores and of "poltophagic" animals, which would indicate that man, to a large extent at least, is naturally a slow-eating animal. Dr. Harry Campbell[30] has also given some evidence, based on a study of primitive tribes, to show that chewing is more thorough among uncivilized races, and that the hurry-habit to which we are accustomed is largely promoted by the use of prepared and "mushy" foods,—which, in fact, appear to have been devised expressly for the purpose of being quickly swallowed.

The evidence, however, on the natural food-habits of man is as yet very meager, and it is only provisionally that we may consider the thorough mastication advocated by Mr. Fletcher as "natural." With this reservation we may say that the experiment here described may be called an experiment in natural eating, or an effort to restore a blunted or lost food-instinct, so that it may serve as a safe guide to the proper quantities and kinds of foods. If it be asked

[28] See *Humaniculture*, N. Y., Stokes, 1904.

[29] See Archiv für Anatomie (Waldeyer's) 1905, p. 321.

[30] "Observations on Mastication," London *Lancet*, July 11, 18, 25 and Aug. 8, 1903. Reprinted in Horace Fletcher's *The A. B.-Z. of Our Own Nutrition*, Stokes, 1903. See pp. 126-135.

in what way this natural eating tended to improve endurance, whether it was because of the finer subdivision of food through mastication; the increased "insalivation"; the increased flow of "appetite juice"; the better adaptation of foods to the particular needs of the individual and the moment; the lessened quantity of food; the lessened protein; or the lessened amount of flesh foods, no satisfactory answer can be given, although, as the previous discussion shows, there is more or less evidence on some of these points. There are certainly some very fascinating problems for the physiologist to solve in regard to fatigue as related to diet. Are the "fatigue poisons" due, for instance, chiefly to the combustion of protein in excess of the physiological needs, as the theory of Chittenden would explain them? Or, are they largely due to the ingestion of these poisons with flesh foods?

While the results of the present experiment lean toward "vegetarianism," they are only incidentally related to that propaganda. Meat was by no means excluded; on the contrary, the subjects were urged to eat it if their appetite distinctly preferred it to other foods.

The sudden and complete exclusion of meat is not always desirable, unless more skill and knowledge in food matters are employed than most persons possess. On the contrary, disaster has repeatedly overtaken many who have made this attempt. Pavlov has shown

that meat is one of the most and perhaps the most stimulating of all foods to the production of gastric juice. Whether the stimulus is natural, or is an improper goad or whip, certain it is that stomachs which are accustomed to this daily whip have failed, for a time at least, to act when it was withdrawn.

Nor is it necessary that meat should be permanently abjured, even when it ceases to become a daily necessity. The safer course, at least, is to indulge the craving whenever one is "meat hungry," even if, as in many cases, this be not oftener than once in several months. The rule of selection employed in the experiment was merely to *give the benefit of the doubt* to the non-flesh food; but even a *slight* preference for flesh foods was to be followed.

Under flesh foods are included all meat- and "stock-" soups. It has been shown that although these extracts of meat contain a large amount of nitrogen, it is not in the form of protein which can be utilized, but only of waste nitrogen which must be excreted. Apparently the sole virtue of such soups is that they supply the gastric stimulus above referred to.

Experience indicates that appetite does not lead to a diet fixed in amount or constituents, but moves in undulating waves or cycles. The men who took part in the experiment were encouraged, after any of the symptoms which seemed to be associated with high protein (such as heaviness, sleepiness, stiffness or soreness after exercise, or catching cold), to cut down on

their protein and substitute fat to restrain the gastric juice. This advice was intended to make application of the theories of Folin[31] that we usually carry a reservoir of protein, enough to supply our needs for body-building for a fortnight. If this reservoir is exhausted, protein starvation occurs and the body feeds on itself; if it is filled too far it overflows and causes the evils of excessive protein. If this theory is correct, the art of eating may consist largely in maintaining a golden mean such that the protein reservoir is neither empty nor overflowing, or at any rate, not overflowing much. Many persons fear to reduce their protein to the Chittenden minimum for fear of protein starvation; but the experience of those who have tried it would seem to show that this fear is groundless, *provided* no violence is done to natural appetite. This may be trusted, so it would appear, to raise a warning in the form of "nitrogen hunger" before the danger point is reached.

The experiment will be seen to harmonize with and supplement the experiment of Professor Chittenden, on which it was founded; but the objects of the two experiments were quite different. Professor Chittenden's was aimed to ascertain the physiological requirements as to protein, and did not touch upon the question of endurance. Moreover, Professor Chittenden, in order the better to measure the protein and nitrogen, artificially reduced the quantities ingested, whereas

[31] "A Theory of Protein Metabolism," *American Journal of Physiology*, March, 1905.

in the present experiment, test was made of Mr. Fletcher's claim, that thorough mastication leads naturally to the adoption of the physiological amount of protein. This we found to be true, especially after the introduction, at the middle of the test, of the suggestion that when appetite was in doubt, the lower protein foods should be selected. But the tendency was quite marked during the first period also, and might have been expected to lead to the same results without the introduction of even the suggestion of voluntary choice, had the experiment been long enough. This was the experience of others, notably Mr. Fletcher himself, whose case, in fact, first called Professor Chittenden's attention to the possible virtues of low protein.

The practical value of the experiment consists in the fact that any layman can apply it, with or without a knowledge of food values, though with more advantage if he possesses than if he lacks such knowledge.

If the dietetic rules of the present experiment are followed, no self-denial as to foods is required. It is, however, absolutely necessary that there should be *self-control* enough to break up the habit of hurried eating to which modern civilization has brought us, habituating us, as it does, to eat against time.

The war now makes it necessary that we in America shall make radical changes in our customary diet. "Food will win the war," and so we are endeavoring

to conserve food. At the same time the Nation demands of every one his best service.

But the question is being asked: Is it possible to keep up the highest efficiency on a reduced diet? Experiments such as the one described in this article give a reassuring answer. They tend to reinforce the Food Administration's dictum, "Eat less, feel better, look better, help win the war." By breaking our unhygienic and even unnatural food habits we shall be led, almost by free choice, to conserve food and, at the same time, gain in endurance. In a rough and ready way, the Nation is now, on a great scale, putting to the proof the contentions of this book.